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Technical Review of the Furniture Fire Model Version 3

Craig L. Beyler

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Technical Review of the

Furniture Fire Model Version 3

by

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Contents

Abstract

This paper reviews the furniture fire model and the documentation of the model in terms of its adequacy, accuracy, generality, and validity. Individual elements of the model are assessed as well as the overall modeling approach.

Serious deficiencies in the model are identified which make it of little value in its present form. Many of the submodels used have not been validated by comparison with literature data, and these submodels differ substantially from well accepted methods in the literature. This brings into question the correctness of the model and its relation to the state-of-the-art. The model has a large number of inputs which are not determined by definite procedures. The documentation of the model is highly fragmented and incomplete. These attributes seriously compromise the validity and usefulness of the model.

Extensive work would be required to make the model useful in hazard evaluations. These include extensive validation of submodels, evaluations of the adequacy of the overall program including experimental and numerical experimentation, and definition of methods for developing the inputs required.

Technical Review of the Furniture Fire Model Version 3

The furniture fire model is intended to describe ignition, flame spread, burning rate, heat release rate, and species generation for burning furniture items. In order to achieve this, extensive modeling of energy transfers to and within the furniture item is also included. It is clear from the documentation that the focus of the effort has been to simulate foam padded chairs and sofas.

The goal of this report is to provide an evaluation of the model in terms of its adequacy, accuracy, generality, and validity. The model's utility as a free standing model and **as** a part of FAST will also be assessed. This report focuses on the model and its documentation. The implementation of the model as a computer code is not reviewed in any detail. The details of the physical/chemical model will first be evaluated, followed by an assessment of the model as a whole.

In general, the review of the documentation will be based on the "Technical Reference and User's Guide to FAST/FFM Version 3." This approach is based on the premise that users should not have to search through prior reports which may include obsolete information to find documentation of features of the current model.

1. Scaling Procedures for Bench Scale Data

Any fire model of this type must rely on small scale test data. The author is to be applauded for including the processing of small scale test data as part of the model itself. However, the author is unclear in his discussion of what tests can provide the required data. This should be quite explicit. It **is** only through my knowledge of the work of Babrauskas/Dipert that I have any sense for the flame spread test **he** appears to be utilizing (referenced in NBS-GCR-86-506). It is unfortunate that the author has not relied on a more widely available test, like the LIFT test.

Organizationally, the documentation of the processing of bench scale test data should properly follow the description of the model itself. It is only in the context of the model that the processing methods make sense. As currently written, much of the model description is included in the bench scale data processing section, leading to confusion.

(a) Surface Heating and Ignition

The ignition of materials is modeled using the ignition temperature concept. Implicitly, it is assumed that all ignitions are piloted. An approximate constant flux heat conduction solution is used (Eqs. **3** and **4)** with heat losses linearized about the ignition temperature.

The approximate heating equations are nowhere compared with more accurate methods. In no case was the heating equation and ignition temperature compared with experimental data. The author asserts that fabric/polyurethane foam combinations are best treated as thermally thin. Again, this is not demonstrated, and the distinction between thick and thin fuels at high fluxes (i.e., $q\sqrt{t}$ = constant versus qt = constant) is not apparent in NBS-GCR-86-506 where the assertion is originally made. This is a good example of the confusion generated by relying on interim reports as references to details. The methods used to solve for surface temperature in NBS-GCR-86-506 are different from those used in the current model, yet the reader is referred there for details. One is left wondering which method **is** actually used and if the fits shown in the interim report are representative of the performance of the current method.

(b) Flame Spread

The author adopts, at least in part, the conventional opposed-flow flame spread modeling approach. The test method and procedure to be used to deduce flame spread properties are not specified, and the data reduction procedures are poorly defined.

In terms of Babrauskas/Dipert style tests, it **is** unclear if flame radiative heating is used to deduce the preheated surface temperature or if the radiative heating **is** included in the flame flux in the numerator of the flame spread equation. In the end, it appears that the author suggests using the FFM to predict the test flame spread rate and select the conductive heating component by fitting. Flame radiative flux to the preheat zone **is** assumed to decrease exponentially with distance, **s,** from the flame. No information is provided as to the determination of the flux at **s** = 0 or the decay length, and examples of reducing flame spread data are not provided. Flame spread is treated in NBS-GCR-**86-506,** but the treatment is different than discussed in the latest documentation. Again, one is left wondering what is actually done and how accurate it is.

The LIFT test is mentioned, but no definite procedure is given for reducing data from this test method. It is not clear why the LIFT flame spread correlation method is not simply adopted entotal. This **is** a well documented and tested procedure. While fabric/polyurethane combinations may not have been tested in the LIFT, it is at least a definite method/procedure.

It does not appear that the FFM includes a module for reducing bench scale flame spread data. Clearly, additional work is needed. What flame spread parameters are needed and how one determines them is wholly unclear.

(c) Scaling Heat and Mass Release Rates

The heat and mass scaling of cone data generally follows the approach of Smith and others. The distinctive aspects here are the stretching parameters in the scaled time, the use of q_o in the heat flux equation, and the use of the concept of a quasi-stoichiometric heat release rate.

The scaled time, which is a weighted cumulative heat release rate, is designed to stretch time near the middle of the test to collapse various flux level tests tu a single normalized time scale. This involves the introduction of three fitting parameters: **d,** ***e,*** and t_{end}. The device appears to be successful, but the importance of d, e, and t_{end} in the success is not clear. Values of d, e, and t_{end} for sample materials are never given. The time stretching **is** also intended to allow variations in material thickness. How this is done is not clear. No correlation of data from different thicknesses is shown, and no explicit dependence of the stretching parameter, G, on thickness or mass is given.

The heat and mass release rates are scaled by the net heat flux to the material. This includes flame convection and radiation, cone radiation, reradiation by the material and q_o, a heat flux fitting parameter. Methods for deducing the flame heat fluxes and material reradiation are not given. Earlier documentation indicated that the flame fluxes are modeled in the same **way** as in the FFM. Since the flame radiation properties are deduced from the cone tests, this makes the process highly convoluted. For reradiation calculations, the surface is assumed black, and a known **burning** surface temperature is assumed, though no method for determining the burning surface temperature is given. The q_o fitting parameter is not well discussed and is presumed to be a material constant, i.e. independent of time and **flux.** NBS-GCR-87-527 gives q_o = **20** kW/m^2 as an optimal value for a particular material. This is quite large, indicating that serious errors exist elsewhere in the model.

The heat release rate per unit area is represented in terms of a quasi-stoichiometric heat release rate. This in effect adds the potential heat release from CO and soot oxidation to the actual heat release rate as measured in the cone calorimeter. This allows the heat release rate to respond to variations in CO and soot generation between the cone calorimeter and a furniture item. However, since no model is used to differentiate the cone and furniture CO and soot yields, this is really just wasted effort, resulting in an unnecessarily convoluted data reduction process and needless complexity.

With all these difficulties, the methods do collapse data at different fluxes to single curves with four fitting parameters (d, e, t_{end}, and q_o). **This** may not be incredible; Smith's method performs reasonably well without these four parameters.

(d) Scaling Species and Soot Generation

The species yields are called mass fraction in the documentation. It would appear that the author **is** not familiar with the nomenclature commonly used in literature.

Species yields are correlated as a function of scaled time. The only validation/demonstration of the method is done based on the ratio of CO to CO, The author claims that the CO and CO_2 are not individually known, but this is impossible in any case where heat release rates are deduced by oxygen consumption methods. The correlations shown are adequate.

The treatment for soot is different and quite ad hoc. The specific extinction area is further scaled using an ad hoc combination of models (Pagni, McCaffrey). This ad hoc model is used to convert the specific extinction area to a soot yield. Hence, it serves the function of providing soot optical properties to allow soot yield to be deduced from an obscuration measurement. The model uses optical measurements made in the flame by Pagni and Bard and ad hoc entrainment assumptions to achieve this. This completely ignores soot burnout and agglomeration processes. No validation is available, and the model has little credibility. The model assumes, among other things, that the soot optical properties in the flame are the same as the soot above the flame (NBS-GCR-88-545).

2. <u>Surface Temperature, Ignition, and Flame Spread</u>

The prediction of thin surface temperatures is performed with a method devised to reduce integration time relative to usual methods. The method is not validated against conventional methods. Thick solids are found using the scheme included in the Harvard Code. It is not clear why different methods are used in the model versus those used in interpreting bench scale test results.

Brief allusion to a pyrolysis temperature and material state is given. It is not clear how this state is modeled and how material properties are to be deduced.

The flame spread model is similar to that discussed in the bench scale test reduction section. Here again, details and validation are lacking. The preheat temperature is taken as the nearest virgin surface element temperature. The flame radiation is assumed to decay exponentially along the distance, **s,** and the maximum flux at s = 0 and the decay constant are not defined. Convection from the flame is assumed constant over a length, l_f. How l_f and the convective flux are chosen is not documented. Under these assumptions, expressions for flame spread rate are developed. The flame spread model is not validated either by comparison with data or other more conventional approaches.

No discussion of concurrent flame spread is included in the documentation. There is reference to the use of a heat flux correlation for vertical surfaces from Quintiere, et al. How and if this is used is unclear.

3. Local Evolution Rates

Local burning rates and heat release rates are deduced from the bench scale correlations using scaled time. The author identifies the scaled time as difficult to evaluate! This incredible statement and the ensuing gymnastics create a problem where none exists. If the scaled time, a historical variable, is difficult to determine, it has no value. It would appear that the author is attempting to use such crude time-step resolution that simple issues become serious problems. This will be discussed more fully later.

The mass loss rate for an element is deduced from the bench scale correlations. It remains to define the heat flux to the material. The local convective flux is deduced from models by Orloff and deRis and by Ahmad and Faeth for horizontal and vertical surfaces, respectively. Radiative fluxes will be discussed elsewhere. The q_o fitting parameter is used as discussed previously.

4. Heat and Mass Release from Adjoining Flame

Individual elements are grouped into flames using methods which are reasonable and logical. This grouping of elements leads to flames which are modeled as parallelepipeds or vertical wedges for purposes of radiation calculations. There is no attempt to model flame merging of burning areas which **are** not contiguous surfaces. (Thomas has developed some simple approaches).

The thickness of the wedge is taken from a correlation by Ahmad and Faeth. The height of the wedge is defined as 40% of the visible flame height as determined by Hasemi's correlation of wall flames. The flame height of the parrallelepiped is taken as 40% of the visible flame height as determined by Cetegen's correlation. Flame heights for merged vertical and horizontal elements are found **by** a weighted linear interpolation between the wall and pool correlation. In the absence of data correlations in the literature, this may be reasonable.

The use of a radiation flame height, which is 40% of the visible flame height, appears to be based on McCaffrey's observation that the continuous flaming region is ~40% of the visible flame height. The author makes the leap of faith that this height is appropriate for radiation calculations. Measurements by Souil et al. indicate that the radiator height should be about *65%* of the visible flame height, not 40% as used in the FFM.

The mass loss, the generation of species, and heat release rate are determined by simply adding up the contributions of each element. This, of course, assumes that each element acts independently, even though the flames from the elements are merged and both CO and soot are formed and destroyed in the merged flame. This points up the fact that the high precision of the cone calorimeter data correlation is really not contributing to the overall accuracy of the model. Scale and flame merging effects are ignored and will have a major effect on the results.

5. <u>Radiative Heat Transfer</u>

Without **a** doubt, the radiative heat transfer portion of the model is the most comprehensive and computationally intensive portion of the model. All furniture surfaces and flame volume are included in the analysis and the model numerically calculates configuration factors. The numerical evaluation **of** configuration factors accounts for a significant portion of the computational effort.

The radiation properties of surfaces are taken as inputs, but methods for their determination are not given. The radiative properties of the flame are determined using rather laborious methods. The gas phase contribution to flame emissivity is done in great detail, though the determinations of CO, and H_2O concentrations in the flame are rather ad hoc. The soot contribution is determined from a user specified $\mathbf{k_{s,max}}$ and the mean beam length correlation of Bard and Pagni. No method for determining $\mathbf{k_{s,max}}$ is given. The flame height is assumed to be 40% of the visible height **as** determined by correlations and the flame shape is a parallelepiped or wedge, depends on the orientation of the surface. **A** uniform flame temperature of 1200K is given, but can be modified by the user. Radiation blockage is not modeled.

Given the very ad hoc nature of the flame radiator properties and flame shape, the use of computational intensive numerical integration of the configuration factors seems excessive and unnecessary. The use of a cylindrical flame and rectangular surface element would allow fully analytical configuration factors to be used which are much less computationally involved and equally valid.

No comparison of the radiation model with experimental data is given, though considerable data is available in the literature. There **is** no demonstration that the computationally intensive radiation model provides better results than can be obtained using more efficient methods.

6. Ventilation Effects

All the modeling done by FFM assumes that the item is burning in the open. While hot layer radiation is included, the effect of the compartment on gas composition of the entrained flow and oxygen availability is wholly absent. This means that generation rates of species like CO do not vary with the equivalence ratio, and the flame radiation temperature does not change with equivalence ratio or lower layer oxygen concentration.

These limitations may be of little consequence for some cases; for many others, they critically affect the hazard. Unfortunately, there does not seem to be any means by which these effects can be introduced by the user. In the current version of FAST, the user can increase CO yields manually. Using the FFM model would prevent such modifications.

7. Documentation

The overall quality of the documentation **is** poor. The most recent report, UDR-TR-89-83 has the correct scope, but the level of detail and clarity is lacking. It is particularly problematic when this document refers to prior reports for details of an algorithm when significant portions of the algorithm have been modified since the earlier report. One is left not knowing what has been retained and what has been replaced.

The documentation of the model is quite incomplete. It would be impossible to reproduce the substance of most of the model subroutines from the description of the algorithms provided in the documentation. I would suggest that in addition to enhancing and clarifying the existing documentation sections it would be useful for the author to follow the lead of the Harvard Computer Fire Code (CFC) documentation which provides summaries of all equations and variables used in a physical subroutine. With the CFC subroutine descriptions, an informed user can always determine what physics are being modeled and how.

The documentation is also quite unclear about what variables are passed from FAST to the FFM and what functions of FAST have been removed and replaced by FFM. For instance, the FFM calculates a heat release rate. Does FAST simply use this value **as** it would for an unconstrained fire, or does FAST impose oxygen availability limitations as it does with the constrained fire? Does the FFM calculate plume **flows** and fuel heights, or are these functions performed by FAST? It is never clear what elements of the normal FAST calculations are performed and which are done by FFM.

The documentation of the required input files is also quite terse and incomplete. The geometric input data file could be discussed in the context of a figure which illustrates the identity, role, and function of the various inputs. The inputs for the bench database should be linked via references to the algorithms which use them and the methods suggested by the author for determining them. In the end, while a great deal of lip service is given to the idea of automated determination of the input parameters from bench scale tests, very little of this is actually implemented. Most inputs to the physical model must be determined manually by the user and by trial and error use of FAST/FFM to calibrate inputs. In the context of predicting the fire growth on a mockup, these many input parameters represent fitting constants. **A** quick review of the bench database file revealed about **20** parameters which must be found by the user.

8. Validation

This model has yet to be truly tested against experimental data. While there have been some comparisons of model calculations with the results of mockup tests (NBS-GCR-89-564), these are very incomplete. Documentation of the methods used to determine the input parameters for these cases and the values used are not given. As noted above, with all the user specified input parameters, any data set can be predicted. This is not to imply any lack of integrity in the comparisons shown, but to point out the lack of detail and recognition of the extent to which the model has been internally calibrated to the tests predicted. It is somewhat disturbing that there are no more recent test results in which bench scale methods and the furniture calorimeter have been used together to provide high quality data suitable for **a** thorough testing of the model. This **is** clearly not something the author has control over, and he needs our support in correcting this omission.

While the lack of validation of the model as **a** whole is disquieting, the lack of validation of component portions of the model is **a** more serious problem. **This** model includes a wide range of phenomena which have been widely studied and reported in the literature. Nonetheless, comparisons of the model with literature data are extremely rare. The only data used for comparisons were generated at NIST by Babrauskas **and** in one case Quintiere et al. Otherwise, the only comparisons **shown** are comparisons of an early radiation model with the model of Dayan and Tien. One **is** tempted to assume that the data used to test the model were thrust at the author. The literature is full of experimental results which should be predictable by the model. These include flame heat transfer, ignition, flame spread, burning rate, and even species generation. There can be no excuse for ignoring the vast literature of results which are available for validation/checking the model components.

Another form of validation of the model has also been ignored by the author: the publication of the work in peer-reviewed journals. It would be inappropriate for the model to be recommended for use without subjecting each of the submodels to the scrutiny of a peer-reviewed journal. I venture to guess that papers based on the work presented in the progress reports would not be accepted in their present form. **A** more thorough, disciplined description of the submodels and some comparison with experimental data would be required. NIST has done itself and the author a disservice by not encouraging the author to publish his work. The quality and credibility of the work product has suffered as a result of this omission. The ability of the model to predict experimental results has never really been demonstrated, and the author has never been required to defend his algorithms in terms of their performance and value over other existing models.

9. Input Parameter Specification

The FFM breaks new ground in the area of input parameter specification. It may well be the first model to incorporate test data reduction and input parameter generation **as** part of the model. The important aspect of this is that the experimental methods/procedures and data reduction methods are fully specified. Perhaps the only other model which has paid close attention to this issue is the OSU model, where the test method came first and the model was developed with the goal of applying the test method results.

Unfortunately, this approach to input parameter determination is restricted to a few parameters which can be determined from the cone calorimeter, such as mass loss, heat release, and species generation. Beyond these, there are approximately **20** fire property parameters which need to be determined by the user. In a few instances, the documentation gives methods for deducing these parameters, e.g. material thermal properties and ignition temperature. For many other parameters, the documentation gives no indication of how the parameters can be found. Still others are explicitly identified as parameters which should be chosen by fitting the model to test data.

The most problematic of the input parameters required of the user are the flame spread and radiation parameters. In most cases, there is really no way to determine these parameters short of fitting the model to test results and using the input parameters as fitting constants. Given the number of parameters to be found in this manner, this can be both impractical and unreliable. There can be multiple (perhaps infinite) sets of parameter value combinations which adequately fit the available data, and one can never know which of these sets will be successful on the intended application.

The author has made a start in this important areas, but much remains to be done.

10. Program Structure

This section discusses the program structure **as** presented in the documentation. As such, it is **as** much concerned with the level of documentation as it is with the program structure itself.

(a) FAST/FFM Interface

The interface between FAST and FFM and the interaction of the two models is not well documented. In particular, the documentation is not explicit about what variables are passed from FAST to the FFM, what variables are calculated and returned to FAST, and how such variable transfers are accomplished.

It is also not made clear what is calculated by FFM and what is left to FAST to evaluate. For instance, FFM calculates a heat release rate. For constrained fires, FAST normally takes this as input data, calculates the entrainment, and determines what fraction of the input heat release rate is actually released. What submodels/options of **FAST** are actually used with FFM is not addressed.

(b) Processing Cone Calorimeter Data

It is not at all clear why the cone calorimeter data reduction is part of the FAST/FFM. In general, it would be more useful if the data reduction were a freestanding utility program which automatically or interactively reduced the data and stored the results in a file readable by FAST/FFM. The current model requires running FAST/FFM with trial values of c, d, t_{end}, and q_o. One then gets out of FAST/FFM and examines the quality of the correlation of the data. One then modifies the parameter guessed and reruns FAST/FFM. This is very cumbersome and inefficient.

In addition, there is ignition data in the current cone data file which is not processed to yield thermal properties and T_{ig}. This is left to the user to do manually. While the test data reduction capabilities of the FFM are limited and the form is cumbersome, the concept is an excellent one and should be continued and improved.

(c) Computational Considerations

As noted by the author, this is a **very** computationally intensive model. This has lead the author to use time steps of 20-40 seconds for the FFM! It is inconceivable that accurate and stable calculations can be performed with this size time step. The early documentation clearly identifies stability problems which seem to have been suppressed by rather ad hoc methods.

The model has not been exercised using a range of time and space descretizations to demonstrate that stable and accurate solutions are in fact being realized. **This,** the most basic form of evaluation of the numerics, must be investigated before the model can be recommended for distribution. Given the methods employed, it is entirely plausible that the solutions are a strong function of the descretization.

(d) Other Program Structure Questions

In reviewing the program flow charts, there appear to be modeling options which are not discussed in the documentation. There does not appear to be any algorithm for downward flame spread on walls or burning algorithms for horizontal downward facing surfaces. The program, as currently structured, does not allow multiple fires and does not support object ignition via heat transfer from other objects. There is no discussion of Hottel's "smoothing factors" in the documentation though they appear in the flow charts. The current implementation does not take into account partial element bum areas in calculating scaled time. Depending on the element size and thickness, one part of the element may have been burned through and out, while another part of the element is not yet involved in flame. The current model cannot deal with such problems.

11. Philosophy/Approach

Developing a furniture fire model is a demanding and time consuming task which of its best draws upon a wide range of prior work from the literature. In this instance, the author has taken it upon himself to generate new models for many of the relevant processes. In all instances, these new models are not compared with existing models and data available in the literature. In doing so, the author missed the benefits of "standing on the shoulders" of existing **work** and the credibility which accompanies the many comparisons of the models with data.

This approach has also meant that less time was available to integrate the submodels, test the resulting larger model against data, and define the limitations of the overall model. By redoing other people's work, the work which only the integrator/modeler can do has received less attention than might otherwise be the case.

The absence of a coordinated experimental program to complement and aid the model development is also had a definitely negative effect on the project. Experiments can and should be performed to make validation of submodels and combinations of submodels reliable and meaningful. Due to the approach taken in model development, there now remains a great deal of work to evaluate and validate the model.

12. Conclusions

In the light of the problems noted regarding the model, the lack of validation, and the lack of definite methods for determining inputs, the FFM cannot be recommended for use in applications at this time. Its principal value at this time is as a research tool. Including FFM in a package like HAZARD would not be a service to the user community.

The course required to make FFM useful in hazard evaluations includes extensive validations of submodels, evaluations of the adequacy of the overall program including experimental and numerical experimentation, and definition of methods for developing the inputs required.

This process should be documented in peer-reviewed journals to assure that the model is widely recognized, and evaluations of the model's credibility and value are documented and available to the potential user audience.

Given the wide array of models which are emerging for many of the submodels, future development would benefit from a modular approach with a well-described program structure. This would allow the inclusion of alternate submodels; much in way this was done in the HAZARD model and as was intended in CCFM. For the foreseeable future, no single submodel is likely to be the best available for all problems. There is still a great deal to be done to allow predictions of fire growth to be made with confidence.

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